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On the Cluster in Perseus. By Dr. O. Pihl.

In the Supplementary Number of the *Monthly Notices* for 1868 was inserted a paper of mine, on the examination of one of the clusters in *Perseus*.

In preparing for publication a detailed account of the subject, I have detected an error in one of the coefficients employed for the reduction of the declinations of the stars, which has caused a systematic error in that co-ordinate, making it too great in some cases by about $1''{\cdot}5$.

Since the paper in question was published, the number of observations has been increased for the declinations of a few stars, which offer peculiar difficulties of observation, and some slight modification in the mean of their declination has, therefore, been obtained.

The following list gives the corrected *relative* values in declination for 85 stars measured, as referred to the star No. 25 in the group:—

No.		No.		No.	
1	+ 759·77	8	+ 816·73	15	+ 1394·77
2	+ 830·24	9	+ 1119·64	16	+ 472·04
3	+ 948·71	10	+ 1380·16	17	+ 1458·96
4	+ 432·36	11	+ 258·30	18	+ 1021·76
5	+ 1250·69	12	+ 300·01	19	+ 806·23
6	+ 354·56	13	+ 1442·02	20	+ 1378·18
7	+ 995·04	14	+ 1387·26	21	− 177·88

No.		No.		No.	
22	= + 805″·59	44	= + 577″·88	65	= + 390″·80
23	+ 895·22	45	+ 700·07	66	+ 319·27
24	− 120·94	46	+ 555·66	67	− 34·93
25	·00	47	+ 384·94	68	+ 480·22
26	+ 1483·48	48	+ 668·70	69	+ 77·33
27	+ 1028·90	49	+ 104·51	70	— 95·94
28	+ 403·40	50	+ 987·12	71	+ 820·11
29	+ 642·24	51	+ 1039·42	72	+ 1096·72
30	+ 440·39	52	+ 1035·67	73	+ 1515·15
31	+ 639·40	53	+ 34·78	74	— 53·93
32	+ 962·33	54	+ 605·16	75	+ 54·65
33	+ 1490·75	55	+ 29·96	76	+ 1304·00
34	+ 287·51	56	+ 598·93	77	+ 930·17
35	+ 708·87	57	+ 34·30	78	+ 719·72
36	+ 716·84	58	+ 1370·13	79	+ 485·43
37	+ 223·61	59	+ 561·17	80	+ 659·27
38	+ 560·53	60	— 1·03	81	+ 742·46
39	− 277·01	61	+ 1158·77	82	+ 114·32
40	− 165·15	62	+ 859·01	83	+ 642·28
41	+ 413·08	63	+ 839·50	84	+ 671·64
42	+ 623·75	64	= + 824·87	85	= + 722·34
43	= + 864·40				

Tullinbjerget, Christiania, 26th July, 1869.

On the Comparative Clinging of the Limb of Venus to that of the Sun in the Transit of 1874 *as compared with that of* 1882. By R. A. Proctor, B.A.

Although Mr. Stone's paper on the above subject in the April Number of the *Notices*, and my own remarks to the same effect in the March Number, perhaps indicate with sufficient exactness the relation between the two transits in the respect dealt with, it may be worth while to determine the actual intervals which will elapse between real and apparent internal contacts for both transits.

I shall adopt Mr. Stone's estimate that about $\frac{1}{60}$th part of the diameter of *Venus* may be allowed for the effects of irradiation (*Monthly Notices* for November, 1868).

Venus in transit (at her rising node) appears to traverse a space equal to her own diameter in $15^m\ 16^s{\cdot}88$, and therefore $\frac{1}{60}$th of her diameter in $15^s{\cdot}28$. This would be in the interval between real and apparent contacts in the case of a central transit.

Now, in 1874 (see p. 216 of my paper in the March Number of the *Monthly Notices*) the chord traversed by *Venus*, from internal

contact most accelerated at ingress to the same phase most retarded at egress, subtends at the Sun's centre

$$360^\circ - (133^\circ\ 56' + 163^\circ\ 27') = 62^\circ\ 37'.$$

Hence the angle at which *Venus's* apparent path is inclined to the Sun's limb, at the epochs of these phases is $31^\circ\ 18'$; and the interval between real and apparent internal contacts is therefore

$$= 15^s{\cdot}28 \times \operatorname{cosec}(31^\circ\ 18')$$
$$= 29^s{\cdot}41.$$

In a similar manner the corresponding interval for internal contact most retarded at ingress, and for the same phase most accelerated at egress

$$= 15^s{\cdot}28 \times \operatorname{cosec}(25^\circ\ 48')$$
$$= 35^s{\cdot}11.$$

The difference, by the way, is greater than one could expect, and shows how necessary it is, in all such cases, to test our views by calculation.

For 1882, I use the position-angles given in a Note in the present Number. I find the interval between real and apparent contacts, for internal contact most accelerated at ingress and the same phase most retarded at egress

$$= 15^s{\cdot}28 \times \operatorname{cosec}(48^\circ{\cdot}3)$$
$$= 20^s{\cdot}46;$$

while for internal contact most retarded at ingress and most accelerated at egress, I find the interval between real and apparent contacts

$$= 15^s{\cdot}28 \times \operatorname{cosec}(45^\circ{\cdot}5)$$
$$= 21^s{\cdot}39.$$

We see, at once, from these values that a given difference of duration will be much more valuable in 1882 than in 1874. Whether an observer in 1874, having from $29^s{\cdot}41$ to $35^s{\cdot}11$ during which to watch the separation of *Venus* from the Sun's limb would make as large a *proportional* error as an observer in 1882 who has an interval of from $20^s{\cdot}46$ to $21^s{\cdot}39$ during which to watch the same phenomenon, may be questioned; that he would make a larger *absolute* error is certain. Assuming the former view we may proceed to compare the time-differences which would have equal values in 1874 and in 1882. Let P be the ratio in which a difference in 1874 must exceed one in 1882, that the two may have equal values; then we must have

$$P^2 = \frac{(29{\cdot}41)^2 + (35{\cdot}11)^2}{(20{\cdot}46)^2 + (21{\cdot}39)^2}$$

(for Delisle's method assumed to be applied to the greatest advantage in either case,—but the ratio is approximately true also for Halley's method). It follows that

$$P = 1{\cdot}547.$$

The ratio I had made use of in the March Number of the *Monthly Notices*, was $50\frac{1}{3} : 32\frac{3}{4}$, which reduces to 1·533, and is appreciably correct; nor does Mr. Stone's value, 1740 : 1219 (or 1·427) in the April Number, differ importantly from the true ratio.* It is worthy of notice, however, how readily a change of the phase considered affects all such proportions. One would have thought that the value which Mr. Stone obtained by considering the intervals between external and internal contact, could not differ appreciably from that which I had obtained by considering the intervals between internal contacts most accelerated and the same phase most retarded by parallax; yet the change is about as from 14 to 13.

Sept. 11th, 1869.

Note on the Transit of Venus in 1882. By R. A. Proctor, B.A.

The elements marked (*) in the following table have been deduced from the formulæ discussed in my paper in the March Number of the *Notices;* the others are Mr. Hind's fundamental elements of the transit:—

	G.M.T. h m s	Position-Angle. From S. to E. °
First External Contact (from Earth's centre)	1 55 38	34·7
*First Internal Contact (most accelerated by parallax)	2 7 51	32·6
„ „ (from Earth's centre)	2 15 56	..
* „ „ (most retarded by parallax)	2 24 18	29·8
		From S. to W.
*Last Internal Contact (most accelerated by parallax)	7 44 5	61·3
„ „ (from Earth's centre)	7 52 27	..
* „ „ (most retarded by parallax)	8 0 32	64·1
Last External Contact (from Earth's centre)	8 12 47	66·1

In the Paper referred to above I expressed my belief "that the corrections which have to be made in the case of the transit

* There is a misprint at p. 221, of the March Number of the *Monthly Notices*, line 13 from the bottom of the page; instead of "observable difference" "maximum difference" should be read. The printer probably took the former reading from the previous sentence. The context shows plainly what was meant; because I immediately apply the maximum differences ($50\frac{1}{3}^m$ in 1874 and $32\frac{3}{4}^m$ in 1882) to the purpose considered. This error, which I have only just detected, serves to explain what had hitherto been quite unintelligible to me, the fact, namely, that Mr. Stone should have written the Paper on the clinging of *Venus* to the Sun's limb without noticing that I had already fully considered the effects of that peculiarity.

of 1882" (when internal contacts are considered instead of passages of *Venus's* centre across the Sun's limb) "are less considerable than those" I had made in the case of the transit of 1874. This view is justified by the calculated positions of the places where ingress and egress are most affected by parallax. These I find to be as follows:—

		Lat.	Long.
(i.)	The place where first internal contact is most accelerated, lies in	° ′ 51 5 S.	° ′ 86 48 E.
(ii.)	The place where first internal contact is most retarded, lies in	53 16 N.	94 28 W.
(iii.)	The place where last internal contact is most accelerated, lies in	26 18 N.	40 1 W.
(iv.)	The place where last internal contact is most retarded, lies in	23 46 S.	137 11 E.

Not one of these places differs by much more than 300 miles from the corresponding place obtained by considering the passage of *Venus's* centre, with the position-angle for external contact,—the phase with reference to which these places had hitherto been calculated.

The transit of 1882 has an interest which that of 1874 wants, in being partially visible in England.

September 11, 1869.

Note on Atmospheric Chromatic Dispersion as affecting Telescopic Observation, and on the Mode of Correcting it. By George Biddell Airy, Astronomer Royal.

In my last communication to the Royal Astronomical Society on the Transits of *Venus*, I adverted to the injurious effect, on the observations, which might possibly arise from the chromatic dispersion produced by the atmosphere. I had carefully observed *Mercury* on the Sun's disk, and I was painfully struck with the colour and consequent indistinctness of the upper and lower limbs both of the Sun and of *Mercury*, and with the prospect of a total loss, in the observations of the Transits of *Venus*, of that delicacy on which the success of the observations will greatly depend: (it being remarked that, for the most valuable observations, the Sun must be low and the atmospheric refraction and dispersion must therefore be considerable; and that the planet must be near the highest or lowest part of the Sun's limb). And I suggested that probably an efficient corrective might be found, in the application of a glass prism of small refracting angle in the eye-piece of the telescope. It is my purpose now to give the numerical elements for the preparation of the necessary prism, and to state the success which attends its use.

The optical theory is exceedingly simple, but it may conduce to clearness that it be given here. Let θ be the angle of incidence when a ray of light falls on a refracting surface; $\theta + \psi$ the angle of refraction; then ψ is the angle of deviation. Also, if the refractive index be $1 + m$, m may be called the index of deviation; m is negative for the passage of light from vacuum to air, and for the passage from air to glass, but positive for the passage from glass to air. At each of these refractions we have the accurate equation,

$$\sin(\theta + \psi) = (1 + m) \sin\theta;$$

and, if θ remain invariable, but m be varied, and ψ be varied in consequence of the variation of m,

$$\cos(\theta + \psi) \times \delta\psi = \sin\theta \times \delta m.$$

Now when ψ is small, but in no other case, $\sin(\theta + \psi)$ is sensibly equal to $\sin\theta + \cos\theta \times \psi$; and in the second equation, the factor of the small quantity $\delta\psi$ may, without sensible error, be considered as $\cos\theta$. Thus, when ψ is small (whatever be the cause of that smallness), the following equations are sensibly accurate:

$$\sin\theta + \cos\theta \times \psi = \sin\theta + m \sin\theta, \quad \text{or } \cos\theta \times \psi = m \sin\theta;$$

$$\cos\theta \times \delta\psi = \sin\theta \times \delta m.$$

From which $\frac{\delta\psi}{\psi} = \frac{\delta m}{m}$; or, the proportion of the change of deviation (accompanying a variation in the colour of the light) to the whole deviation, is the same as the proportion of the change of the index of deviation to the whole index of deviation.

The two deviations which require consideration in the following investigation, namely, that produced by the atmosphere and that produced by a flint prism of small angle, are both small; the first because, though the angle of incidence is large, the index of deviation is very small; the second because, though the index of deviation is large, the angle of incidence is small. The theorem just found will, therefore, apply to both.

In collecting the numerical data to be employed for my calculation I have been referred by my friend Professor W. H. Miller to an account of Ketteler's observations on atmospheric refraction, given in the *Fortschritte der Physik*, 22nd Jahrgang, page 179. For the other media I use some well-known determinations by Fraunhofer. Although, in the practical construction, I use only flint glass, I have thought it interesting to insert the numbers for crown glass and water. The two rays of the spectrum by which I measure the dispersion are B and G.

Medium.	Index of Deviation for B.	Index of deviation for G.
Air ...	0·00029350	0·00029873
Water ...	0·0330977	0·0341261
Crown 13 ...	0·0524312	0·0539908
Flint 30 ...	0·0623570	0·0655406

Now dividing the difference of the indexes of deviation for B and G by half their sum, for each medium, we obtain the following values of $\frac{\delta m \text{ (B to G)}}{m}$:—

Air	...	0·017662,
Water	...	0·030596,
Crown 13	...	0·029310,
Flint 30	...	0·049784.

Air, therefore, is much less dispersive in proportion to its refraction than either of the other media.

I now proceed to apply these numbers to the estimation of the effect of atmospheric dispersion on telescopic vision, and to the construction for its correction.

Let R be the atmospheric refraction of the object viewed; p the power of the telescope. Then the atmospheric dispersion (B to G), as seen by the naked eye, is $R \times 0{\cdot}01766$; and, as seen in the telescope, it is $R \times p \times 0{\cdot}01766$. A prism of flint glass is to be introduced close to the eye, whose dispersion is to neutralize this magnified atmospheric dispersion. Therefore the dispersion of the flint prism (B to G) must be $R \times p \times 0{\cdot}01766$; its refraction must be $R \times p \times 0{\cdot}01766 \times \frac{1}{0{\cdot}04978}$; and, since the index of deviation of the flint glass for the mean rays is 0·6395 (estimated as for the passage from glass to air, which, it is easily seen, give the proportion of the aggregate of deviations at the two surfaces to the refracting-angle of the prism), the refracting angle of the prism must be $\frac{R \times p \times 0{\cdot}01766}{0{\cdot}04978 \times 0{\cdot}6395}$. Performing the numerical calculation, we find that the angle of the flint prism must be $\frac{R \times p}{1{\cdot}8}$, or the atmospheric refraction must be $\frac{1{\cdot}8 \times \text{angle of the flint prism}}{p}$, in order to produce perfect correction.

Suppose, then, that we have a series of flint prisms ground to the angles 2°, 4°, 6°, 8°, 12°, 16°. And suppose that we use a telescope with power 120 or with power 240. Then the following table, showing the zenith distance at which the atmospheric dispersion is corrected, is easily computed; the refraction being calculated by the formula just given, and the zenith-distance corresponding to the refraction being taken from a common table of refractions:—

Angle of Flint Prism.	Telescopic power 120. Atmospheric Refraction.	Zenith Distance.	Telescopic power 240. Atmospheric Refraction.	Zenith Distance.
°	′	° ′	′	° ′
2	1·8	61 58	0·9	43 7
4	3·6	75 16	1·8	61 58
6	5·4	80 15	2·7	70 32
8	7·2	82 52	3·6	75 16
12	10·8	85 34	5·4	80 15
16	14·4	87 3	7·2	82 52

For view with the naked eye it would be necessary to use a prism (of appropriate small angle) with its edge downwards; but, for view with an inverting telescope, the edge of its appropriate prism must be upwards.

Mr. Simms has prepared, under my instruction, a series of prisms of flint glass with the angles just mentioned; and Mr. Carpenter has made trial of them. The following are two of Mr. Carpenter's reports :—

1869, July 15, $11^h\ 50^m$ to $12^h\ 20^m$. *Saturn*, zenith-distance about 82°, viewed with the Great Equatoreal, power 245. Atmospheric dispersion very bad.

Prism 12° corrected the dispersion perfectly.

Prism 8° corrected it fairly.

Prism 6° corrected it partially.

Prism 4° and 2° produced no visible effect.

Prism 16° confused the image.

1869, July 21, 9^h. The Moon, zenith-distance about 78°, viewed with the Altazimuth, power about 100.

Prism 2° slightly diminished the colour.

Prism 4° better than the last.

Prism 6° destroyed the colour entirely. The definition of the details about the Moon's rugged edge was much improved.

Prisms 8°, 12°, 16°, gave an opposite dispersion.

It will be seen here that the object which I proposed to myself is completely attained. It is made possible, by this construction, to examine a celestial body with delicacy and accuracy, under circumstances which would, without this construction, have rendered nice observation impossible.

The series of angles of the prism which I have given appears to me well adapted to general wants. I propose to furnish each of the principal telescopes to be used for the Transit of *Venus* with a complete series of such prisms, arranged perhaps on a long slider. Care must be taken to make the thickness of the slider-frame as small as possible, inasmuch as it must be accompanied with another slider carrying dark glasses. It will probably be

found best to place both sliders between the two glasses of the eye-piece. This slightly disturbs the elements of the calculation above; but in practice the selection of the best prism will always be matter of trial, and the disturbance of calculations will be unimportant.

No assistance is given by this principle to the accurate determination of zenith-distances. The construction, which destroys the prismatic extension of a star's image, necessarily introduces an equal prismatic extension in the opposite direction on the breadth of the wire.

Before closing this subject I will advert to a remark made by one of the most acute telescope-observers who have ever been known in this Society, the late Rev. W. R. Dawes. He states that, in general, a telescope performs better with one particular point of the edge of its object-glass upwards than in any other position. The explanation of this singular remark will be found, I think, in the combination, of the effect of error of centering of the two lenses of an achromatic object-glass, with the effect of atmospheric dispersion. The centre of one lens (using the word "centre" to denote that part in which the tangent-planes of the two surfaces are parallel) ought to be exactly above the centre of the other lens. But it is not easy to make this adjustment perfect; the centre of one lens is frequently above a part of the other lens where the two surfaces have a slight inclination; and the refraction thus created produces in the image of every star a spectrum which rotates as the telescope-tube is made to rotate. In one position of the tube the atmospheric dispersion is opposed to this, and may wholly or in a great measure correct it; in the opposite position the atmospheric dispersion is added to it, and increases its injurious effects.

The atmospheric dispersion between B and G is about $\frac{1}{60}$th of the atmospheric refraction. At zenith-distance 45° it is nearly 1″, at 63° it is nearly 2″, at 80° about 5″. These are the lengths of the visible spectrum.

Royal Observatory, Greenwich,
1869, August 7.

Distribution of the Nebulæ. By R. A. Proctor, B.A.

Rather more than two years ago a paper by Mr. Cleveland Abbe on the distribution of the Nebulæ in space was read before this Society. That paper included a valuable contribution to science, in the form of a tabular statement exhibiting the manner in which the irresolvable Nebulæ are distributed over the celestial sphere. A few weeks before I had sent to the editor of the *Student* a paper on the same subject. Until this paper and four others dealing with the distribution of matter throughout that

portion of space which falls within our cognisance had appeaerd, I did not feel at liberty to bring before this Society certain views which have been for a long time in my thoughts. Owing to the press of other matter, and especially of other astronomical matter in the *Student*, the series of papers was only completed last April. This is, therefore, the earliest opportunity I have had of submitting the views I speak of to the judgment of the Royal Astronomical Society.

The four pairs of maps now laid before the Society have been formed from the valuable table prepared by Mr. Abbe; but it is worthy of notice that their teachings differ in no important respect from those conveyed by maps formed from a similar table prepared by Sir John Herschel many years ago. It is important that this should be attended to; because (1) Herschel's table was formed from a catalogue of only 3812 nebulæ, while Mr. Abbe's was found from a catalogue of 5079 nebulæ; (2) Herschel's tabulation corresponded to a division of the sphere into spaces of 1^h in R.A. by $15°$ in Decl., while Mr. Abbe's took spaces of only 30^m in R.A. and $10°$ in Decl.; and (3) Herschel included all nebular objects (if I remember rightly) in his tabulation, while Mr. Abbe drew a distinction between the various orders of nebulæ, so that his table as finally drawn up included only 4053 irresolvable nebulæ. When we find that all the refinements thus introduced by Mr. Abbe, and an increase of nearly one-third in the total number of objects dealt with, produce no appreciable change in the indications which the nebulæ present of being aggregated together according to certain definite, and, as I think, very significant laws, we seem free to conclude that no extension of telescopic observation can appreciably affect our views respecting the distribution of the nebulæ. If the second tabulation had indicated laws appreciably different from those which the first had exhibited, the conclusion would have been that we must wait for an increase of knowledge before venturing to theorise; as it is, we may proceed to deal with the facts before us with some confidence as to the general accuracy of their indications. We see, then, that the two tables have an increased value when considered together.

I would first call attention to the pair of maps numbered I. The plan of projection is the isographic projection described in my *Handbook of the Stars*.* The meridians and parallels are so drawn as to correspond to Mr. Abbe's tabulation, and in each space as many nebulæ as his table indicates are marked in, so as to spread pretty uniformly over the space. Thus the projection being isographic, the maps represent the actual distribution of the nebulæ (as to density of aggregation, &c.) over the northern and southern hemispheres.

Now when this is done we see that there is much more in the

* After that book was published I found that Sir J. Herschel had already suggested this construction. The coincidence is not very surprising when it is remembered that the problem of a central isographic projection is a very simple one, and has only one solution.

arrangement of the nebulæ than that thinning off in the neighbourhood of the Milky Way which Mr. Abbe had recognised. No one can look at the general aspect of the two maps without seeing that the suggested distribution of the nebulæ in a figure roughly resembling a prolate spheroid, having the plane of the Milky Way at right angles to its longer axis, is wholly insufficient to account for the actual aspect of the maps. The northern map accords better with this view than the southern; but even in this map there is an irregularity in the clustering, an occasional evidence of streaminess, and in places a sharpness in the outline of the group which is altogether different from what would be seen if the nebulæ formed a figure even roughly resembling that suggested by Mr. Abbe. In the southern map all these features are exaggerated. In this map also a new and unexpected feature strikes us. We see that the Nubeculæ are associated by well-marked nebular streams with the main southern group.

But now it is obvious that the pair of maps numbered I. requires to be supplemented by maps which shall exhibit—(1) the two groups of nebulæ in their entirety, and (2) the great zone bare of nebulæ which divides one group from the other.

The pair of maps numbered II. is intended to satisfy condition (1) as nearly as can be done on equatorial maps. The projection corresponds to that used in maps I.* The first map of the pair exhibits the nebular group to the north of the Milky Way, the second exhibits the nebulæ to the south of that zone. It will be noticed that the contrast between the two groups is very striking.

The northern group presents a somewhat stratified aspect, and we can understand how it was that Sir W. Herschel, observing only in northern latitudes, conceived that the nebulæ might be found to be arranged in a zone somewhat resembling the Milky Way, but nearly at right angles to it. On a closer inspection, however, we recognise in the northern cluster a decidedly streamy character.†

* So far as I am aware the pairs of maps numbered II. and III. are the first in which the relations of the nebulæ have been presented isographically on any but a polar projection.

† It is to be noticed that it would be perfectly fair to strengthen the indications of these maps, by spreading the dots which represent the nebulæ less regularly over the spaces they belong to. At first sight this seems illegitimate. It might be argued, for example, that to take the streams presented to us on a first inspection, and to add to their obviousness by a special arrangement of the dots in the figure, is nothing less than to adopt an hypothesis, and to arrange our facts so as to seem to bear it out. But in reality this is not so. The process of arrangement so as to intensify the aggregations of dots and to make the vacuities more clearly apparent corresponds exactly to the process by which, having a series of ordinates in any graphic construction, we take a *curve* through their extremities instead of a series of rectangular cross-lines. The curved lines, in this case, slope upwards towards the direction of the longer ordinates, and downwards towards the direction of the shorter; and precisely in the same way the dots in any of the spaces in such maps as we are now dealing with should aggregate towards the direction in which there are greater aggregations, and be more

The southern group of nebulæ presents features of a very interesting character. As in the northern group, the general aspect of the system is streamy, but this character is much more marked in the southern group. A peculiarity is also noticeable in the fact that the most striking aggregations are at the *extremities* of the streams. In fact, the central part of the whole group, a region exactly opposite the great northern aggregation of nebulæ, is almost vacant. We have, again, distinct evidence of the connexion between the Magellanic Clouds and the southern nebular group. The Nubeculæ are in fact merely instances, more marked than the others, of the tendency to aggregation at the extremities of the nebular streams which form the great southern group.

I now turn to the pair of maps numbered III., which serve to bring out more especially the great vacant zone. This I hold to be the most significant of all the features presented by the nebular system. It will be observed that there is no mistaking the fact that a true zone of nebular dispersion (if I may use that term as the converse of aggregation) exists in the heavens. Nor can the exact agreement of this zone with the galactic zone be called in question. I would now ask whether the conclusions to be drawn from these peculiarities ought not to be somewhat different from those which have been commonly accepted.

In the first place, I would remark that Sir W. Herschel was disposed to look on the position of the zone of nebulæ, which he supposed to exist in the heavens, at right angles, or nearly so, to the Milky Way, as not accidental. The peculiarity we actually have to deal with, however, is much more significant; first, because the agreement of two zones in position is a circumstance which is antecedently much more improbable than mere perpendicularity of intersection; and secondly, because the existence of a zone of dispersion is in itself a much more remarkable circumstance than the existence of a zone of aggregation. On the first point, it is merely necessary to remark that to secure perpendicularity between two zones it is requisite only that the poles of one zone should lie upon the other, which can happen in an infinite number of ways; whereas, to secure coincidence, the poles of one zone must coincide in position with the poles of the other, which can happen only in one way. On the second point, it may be noticed that the existence of a zone of dispersion would be very remarkable even if the zone were not particularly well marked;

sparsely strewn in the direction towards which there are vacuities. Had this been done the streaminess already clearly apparent in the arrangement of the nebulæ would have been rendered much more obvious. In the large diagram* I have felt justified in following this plan. Indeed so far from being unfair, it may be shown to be that which, according to the doctrine of chances, gives the arrangement which would most probably be observed if each nebula had been marked in its proper place.

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* This diagram was exhibited at the Meeting.

because all that we know of the universe leads us to look upon systems aggregated into the figures of cylinders, prolate spheroids, or the like, as much less likely to be met with than systems in the figure of disks, rings, oblate spheroids, or the like; and it is only figures of the former sort which can result in showing a zone of dispersion, figures of the latter kind exhibiting (as in the case of the sidereal system) a zone of aggregation. But a very well-marked zone of dispersion, as in the case of the nebular system, is a yet more significant phenomenon, because it can only be explained by looking upon the aggregations between which it lies as distinct systems. Looking on the two nebular clusters in this light, the fact that the Milky Way agrees exactly in position with the zone which separates them, becomes one which we are bound to account for in forming any theory of the nebulæ. In fact, when dealing with the aggregation of nebulæ near the north pole of the galactic system, Sir John Herschel expressed this very opinion; though he has nowhere indicated, so far as I am aware, the conclusion which seems to me to follow directly from that opinion:—

I cannot see how we can look upon the coincidence I have spoken of as *not* accidental, without being led to the conclusion that the nebular and stellar systems are parts of a single scheme. If the nebulæ were external star-galaxies, the coincidence might have happened, but it would be accidental. If they formed a system "distinct from the sidereal system, though involving, and perhaps to a certain extent intermixed with the latter," as Sir John Herschel suggested, the coincidence would still be accidental. If "our system" (again to quote Sir J. Herschel) "lies outside the denser part of the cluster, but is involved within one of its outlying members," or "forms *an element* of some one of its protuberances or branches," yet, again, the coincidence would be accidental. No theory, which looks upon our galaxy as simply a member of the nebular system can possibly be reconciled with the view that the position of the zone of nebular dispersion is otherwise than accidental, unless we assume either (i.) that *all* the members of the system, or at least all in our neighbourhood, have a position parallel to that of our galaxy, or at right angles to the length of the nebular system, or (ii.) that there is some peculiarity in the galactic stratum preventing us from looking so far out into space along its length than elsewhere. The first supposition is disposed of by the briefest study of the nebulæ, which (looked upon as disk-shaped universes) have every variety of position. The second has a greater appearance of probability, yet it would be easy to dispose of it on its own merits. It must be dismissed, however, for a reason associated with a matter which yet remains to be considered.

We have seen that the nebulæ belonging to the Magellanic clouds are associated in a very obvious manner with the rest of the nebular system. But it must not be forgotten that these strange objects are also intimately associated with the sidereal

system. In fact, their visibility to the naked eye is due entirely to the fact that they contain a number of small stars. Here, then, the sidereal and nebular systems are seen as they are nowhere else to be observed, *intermixed.* Perversely enough, this peculiarity, which seems so obviously to indicate an intimate association between the two systems, has been held by many distinguished astronomers, to prove that the Nubeculæ belong neither to the sidereal nor to the nebular system. Sir John Herschel has distinctly pointed out the conclusions which should be drawn from the evidence given by the Nubeculæ; yet he nowhere definitely rejects views to which those conclusions seem directly opposed. He simply remarks that we may be led to look with some suspicion upon views which "in former pages had been somewhat positively insisted upon." *

The evidence afforded by the Nubeculæ seems to me decisive in favour of the intimate association between the stellar and nebular systems, which I have been endeavouring to establish. When it is combined with the evidence before adduced, no reasonable doubt can exist, I think, that the stars and nebulæ are members of the same scheme. There may be individual nebulæ which are true external universes,—It is possible, for example, that the spiral nebulæ may be of this class, as also perhaps the *Andromeda* nebula, and a few others,—but I consider that for one nebula which is really external there are hundreds which are associated with the sidereal system.

What the processes may be which have led to the present arrangement of the great scheme of stars and nebulæ, I shall not venture even to conjecture. I may remark, however, that the great majority of the nebulæ constituting the northern and southern clusters may be looked upon as owing their present constitution to the fact that they are outside the region of most active stellar aggregation, and not to any difference in their original structure. I believe that the irresolvable and scarcely resolvable nebulæ are composed of stars really smaller and really closer than those forming the clusters and easily resolvable nebulæ. In favour of this view I would adduce a peculiarity exhibited in Mr. Cleveland Abbe's tabulation of the nebulæ, the true meaning of which seems to have escaped him.

* I may remark here, in passing, that the tentative and philosophical tone in which Sir John Herschel has reasoned respecting the universe, the light grasp with which he holds the theories he has himself developed, and the wide range over which his views extend have, on the one hand, encouraged me to adopt views opposed to those which he has sanctioned by his approval, and, on the other, to put forward views which he has already mentioned, but to which I have been led by independent considerations. His whole treatment of the subject of the sidereal and nebular systems indicates that the views he has put forward are intended for the most part rather as suggestions than as theories. This power of weighing evidence without reference to preconceived opinions; of indicating the theories suggested by this or that portion of the evidence, and as readily indicating reasons which must lead to the abandonment of such theories, may be looked upon as characteristic of the Herschel family. It would be well for the cause of scientific progress if the power were not so uncommon as it is.

If the objects in Sir J. Herschel's Catalogue are arranged in the following order,—(i.), clusters; (ii.), easily resolvable globular clusters; (iii.), resolvable globular clusters; (iv.), resolvable nebulæ; and (v.), irresolvable nebulæ, and the arrangement of these classes of objects with reference to a galactic zone 10° wide be considered, we find that objects in class (i.) are obviously aggregated on the galactic zone; objects in class (ii.) less so; those in class (iii.) still less so; those in class (iv.) are aggregated away from the galactic zone; and those in class (v.) are aggregated almost exclusively outside that zone. When we take a galactic zone 30° wide, we have,—objects in class (i.) almost exclusively on the galactic zone; those in class (ii.) decidedly aggregated there; those in class (iii.) somewhat less so; while those in the other classes present much the same relations as before.

This plainly shows that the first three of these classes are associated with the galaxy in proportion to their resolvability; the fourth class, which is still resolvable, is removed from the neighbourhood of the galaxy; and the fifth still more so. Now, can we look upon the distinction between the resolvable nebulæ and the resolvable clusters as so marked that, whereas we can admit the latter to be associated with the galaxy (or rather we are forced to do so), we must look upon the former as lying far away beyond the range of the sidereal system? But even if we must do so, how can we then account for the fact that the irresolvable nebulæ show a greater antipathy to the neighbourhood of the galaxy than their resolvable fellow-systems? If we do not, if we look on the resolvable nebulæ as belonging to the sidereal scheme, notwithstanding their aggregation away from the galactic zone, what reason can we assign for forming a different opinion of the irresolvable nebulæ which exhibit precisely the same relation, only in a more marked form? Finally, if we admit the irresolvable nebulæ into the galactic scheme, we cannot look upon greater distance as accounting for their irresolvability, because once they are recognised as associated with the galaxy, we can see no reason for the more distant members of the nebular part of the universe lying towards the poles of the galactic zone. The antecedent probability of such a relation prevailing, that *distance* is the true cause of the irresolvability of the objects forming the great nebular clusters, is exceedingly minute. Of course, *cæteris paribus*, the more distant a cluster is, the less easily resolvable it will be. But we must look for some other cause than distance to account for the fact that nebulæ seen outside the milky way are less easily resolvable than nebulæ seen upon that zone. I believe that cause may be assumed not unreasonably to be the difference in the circumstances under which the galactic and the extra-galactic nebulæ have reached their present state.

I have hitherto omitted all reference to the gaseous nebulæ. So far as present observation goes, these objects show a decided preference for the neighbourhood of the galactic zone. The planetary nebulæ, all of which are probably gaseous, are so distri-

buted, that two-thirds are on the wider galactic zone referred to above, whose area is less than one-fourth that of the whole sphere. The irregular gaseous nebulæ are all on or close to the Milky Way, except one, which is in the greater Magellanic Cloud; another proof, if any were wanting, that the Nubeculæ are not external systems.

In the pair of maps numbered IV. I have combined the visible sidereal system with the nebular scheme; on the same isographic projection, as in maps I. My object was to discover, if possible, whether any laws can be discovered associating the nebular and sidereal systems together. I notice that, outside the Milky Way, the nebulæ seem to be associated with the lucid stars in this way, that, where there are many leading brilliants, there nebulæ seem densest. It is to be noticed that there is not a single marked vacancy in either of the nebular groups where there is not at the same time a marked absence of bright stars. I would point out, also, that, of the two most marked nebular streams, one is coincident with the stellar stream recognised by the ancients in their figuring of the constellation *Eridanus* (the prolongation southwards being recognised by modern astronomers in the constellations *Hydrus* and *Reticulum*); the other coincides with the stellar stream associated by the ancients with the water from the vase of *Aquarius*. This association between stellar and nebular aggregation seems to occupy a character midway between the yet closer relation observed in the Nubeculæ, and the directly contrary relation observed in the neighbourhood of the Milky Way.

On a Method of Determining the Dimensions of the Disks of those Stars which are liable to be Occulted by the Moon. By Richard A. Proctor, B.A.

The following plan for measuring star-disks is theoretically sound, but may probably present great difficulties in practice. It seems worth being tried, however.

If the image of a star is put in rapid rotation (which may be done in many ways) while the centre of rotation moves in a circle, in a period exceeding the maximum duration of luminous impressions on the retina, the appearance presented to the eye will be that of an epicycloidal coil of light, whose rapidly advancing end is always on a black ground. Now if the eye be intently fixed on this coil just before the star's occultation by the Moon, it is obvious that at the instant of occultation the whole coil will vanish; but, during the brief interval which the luminous impression on the retina occupies in vanishing, there will be time to notice the appearance of that end of the coil which was last formed. Now if we suppose the rapidity of rotation such that a quarter of a revolution was made while the Moon was traversing

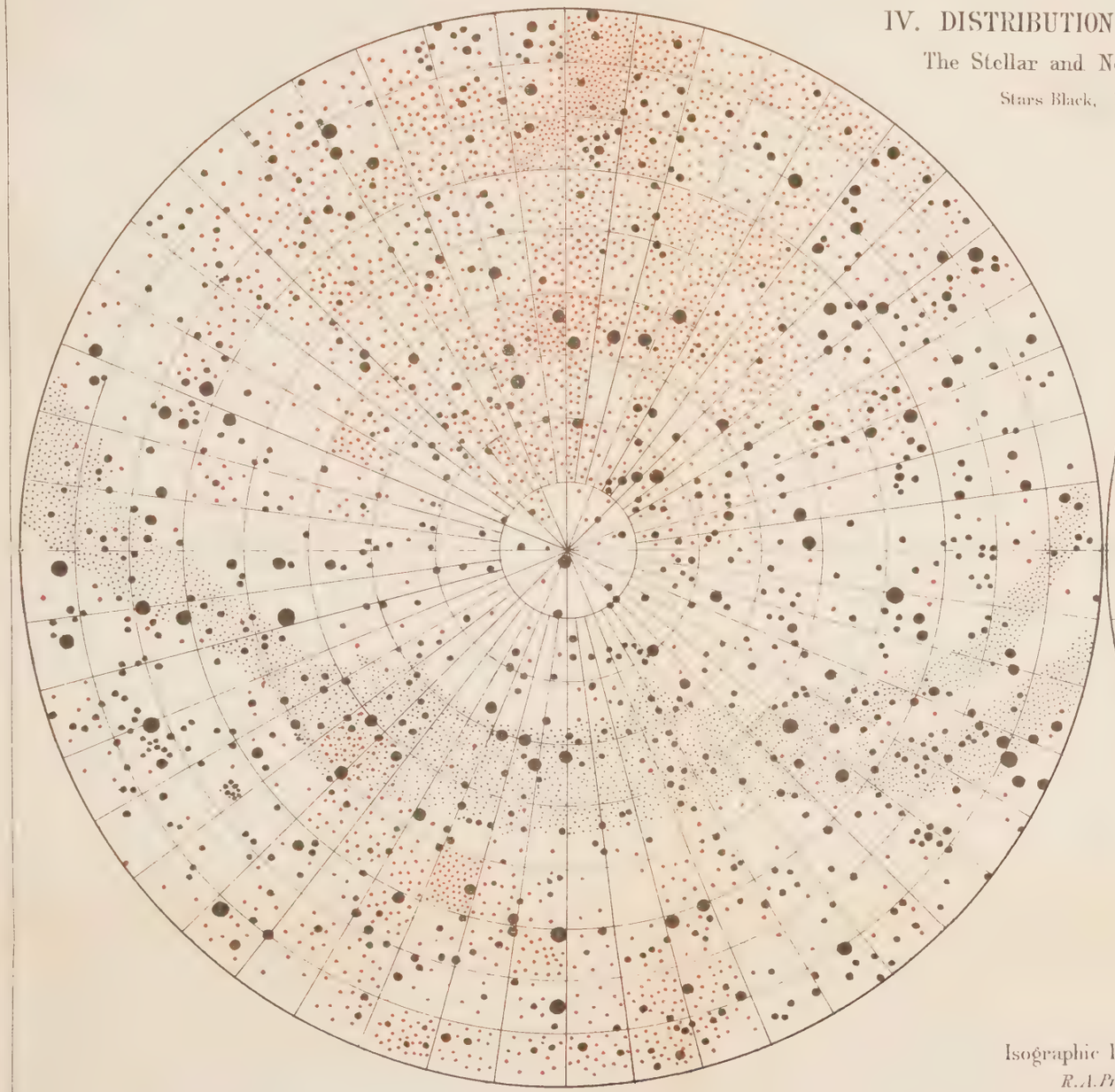
NORTHERN HEMISPHERE.

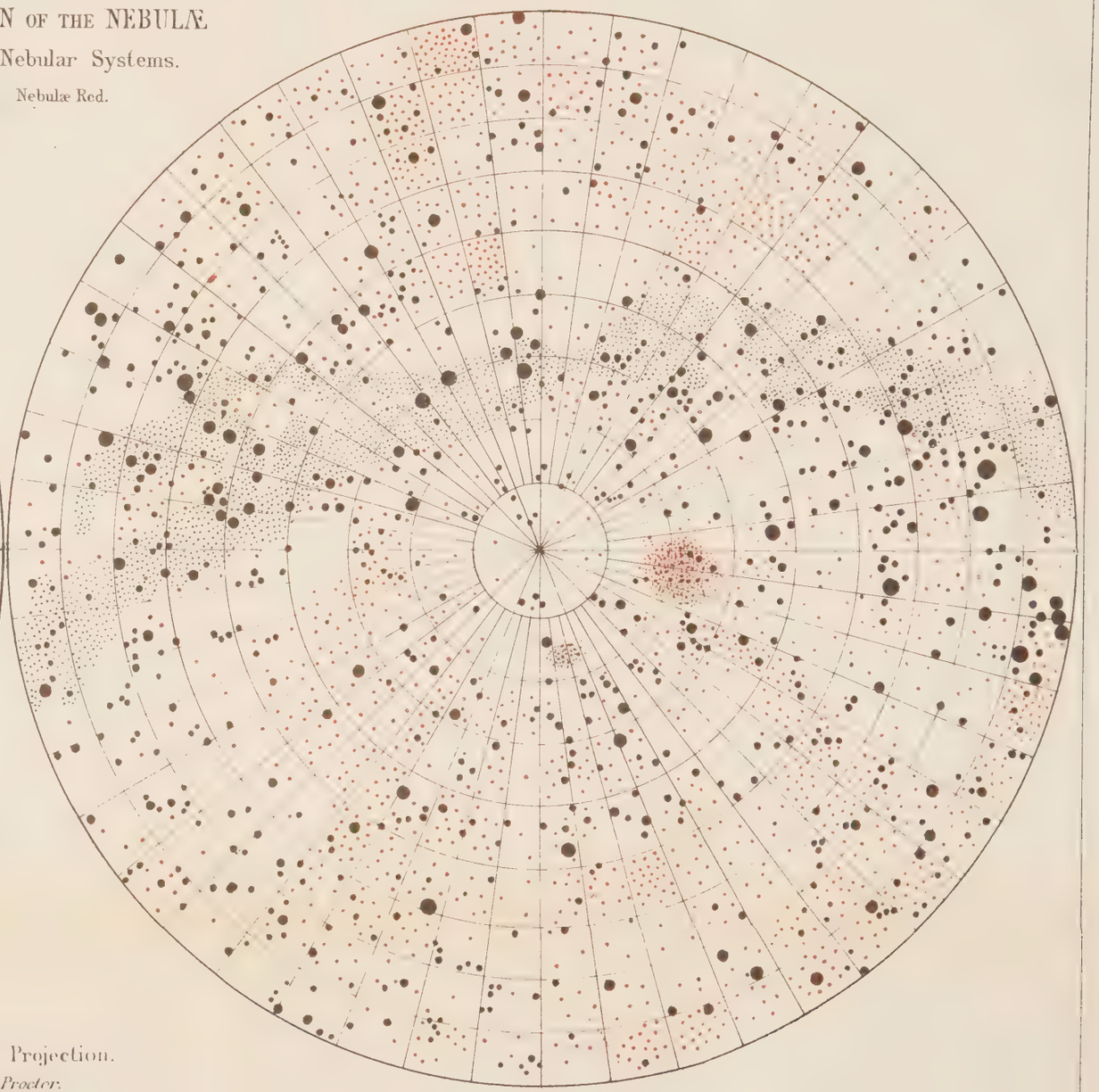
SOUTHERN HEMISPHERE.

IV. DISTRIBUTION OF THE NEBULÆ

The Stellar and Nebular Systems.

Stars Black, Nebulæ Red.



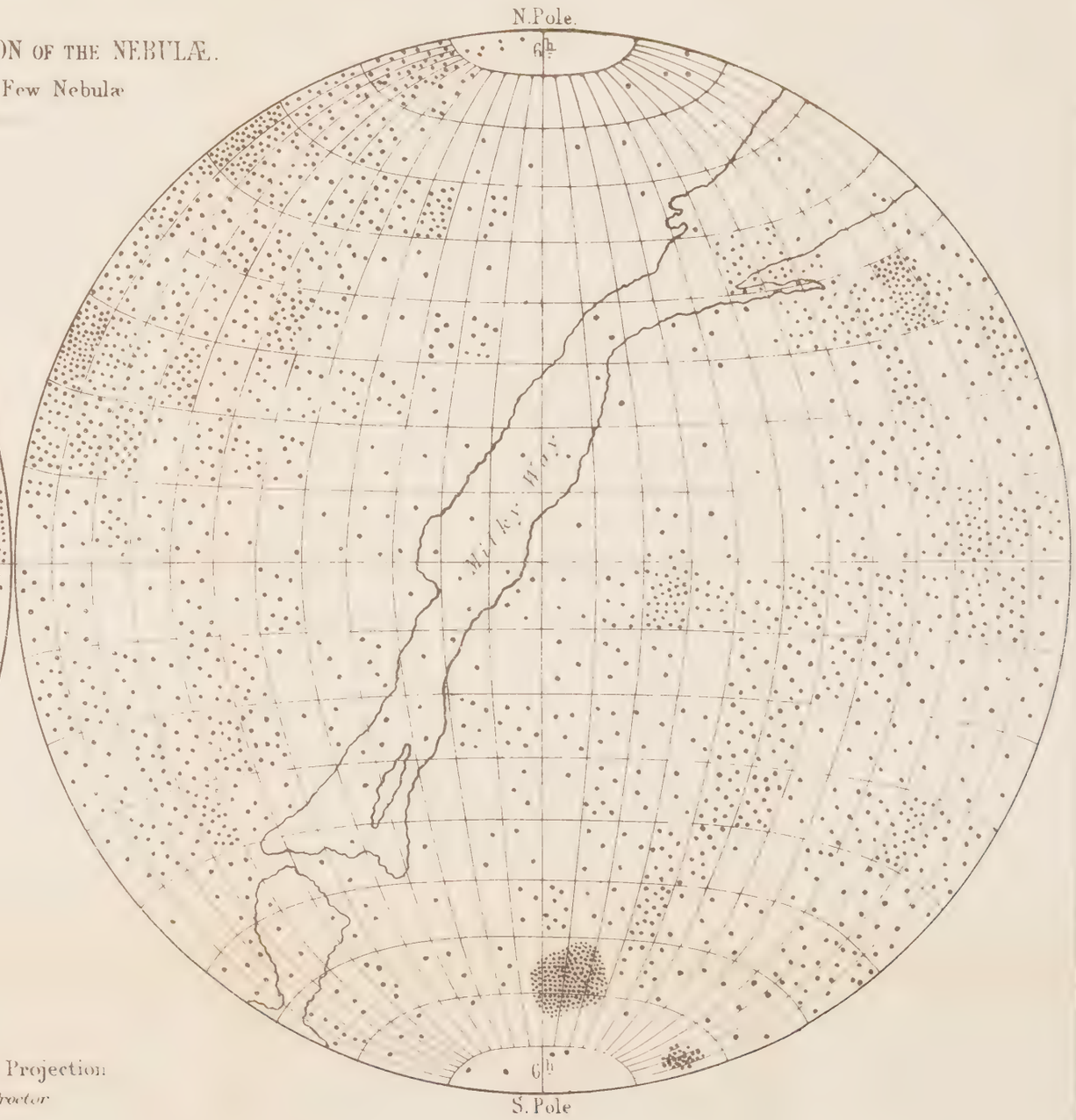


Isographic Projection.

R.A.Proctor.

III. DISTRIBUTION OF THE NEBULÆ.

The Zone of Few Nebulæ

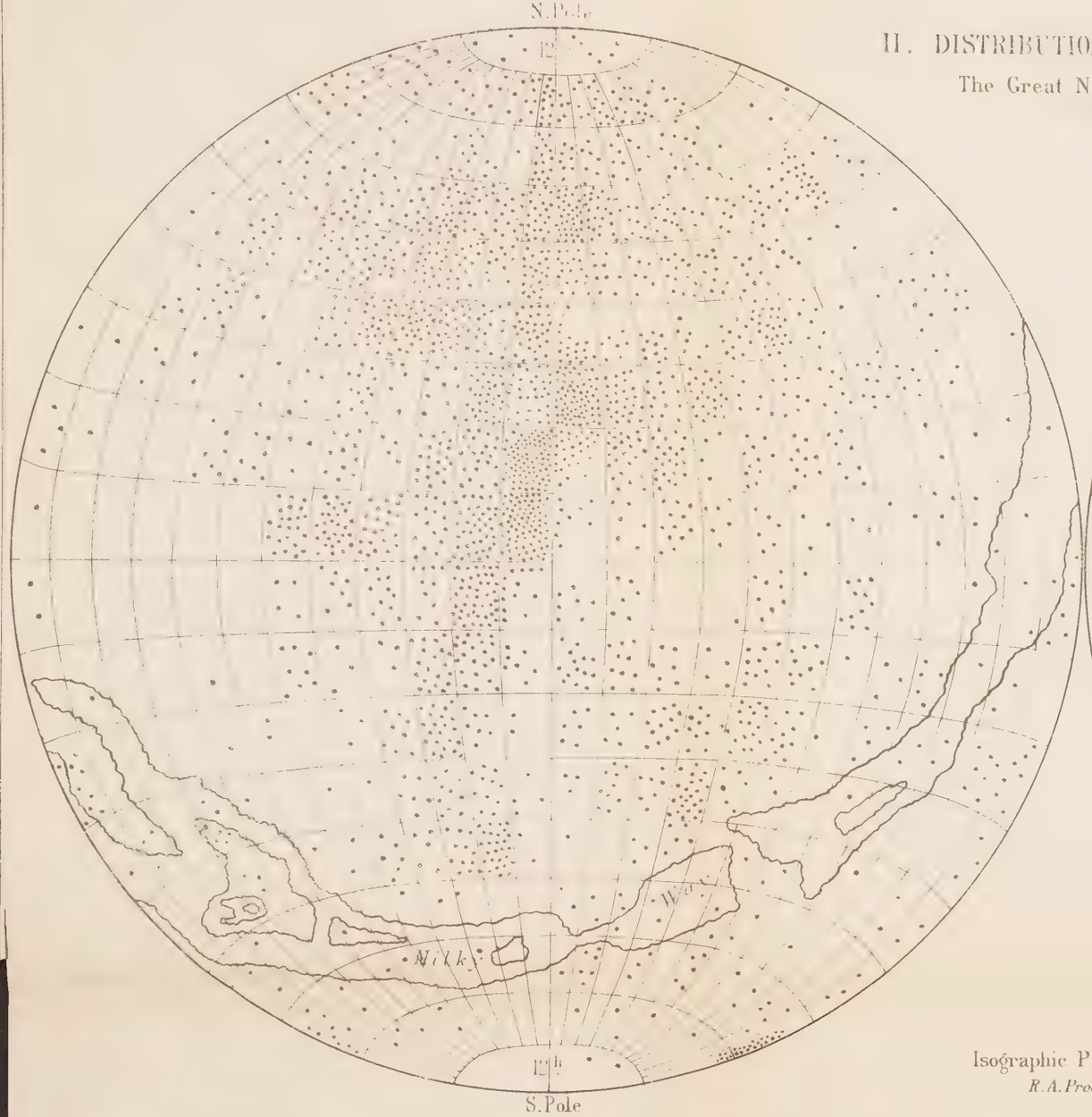


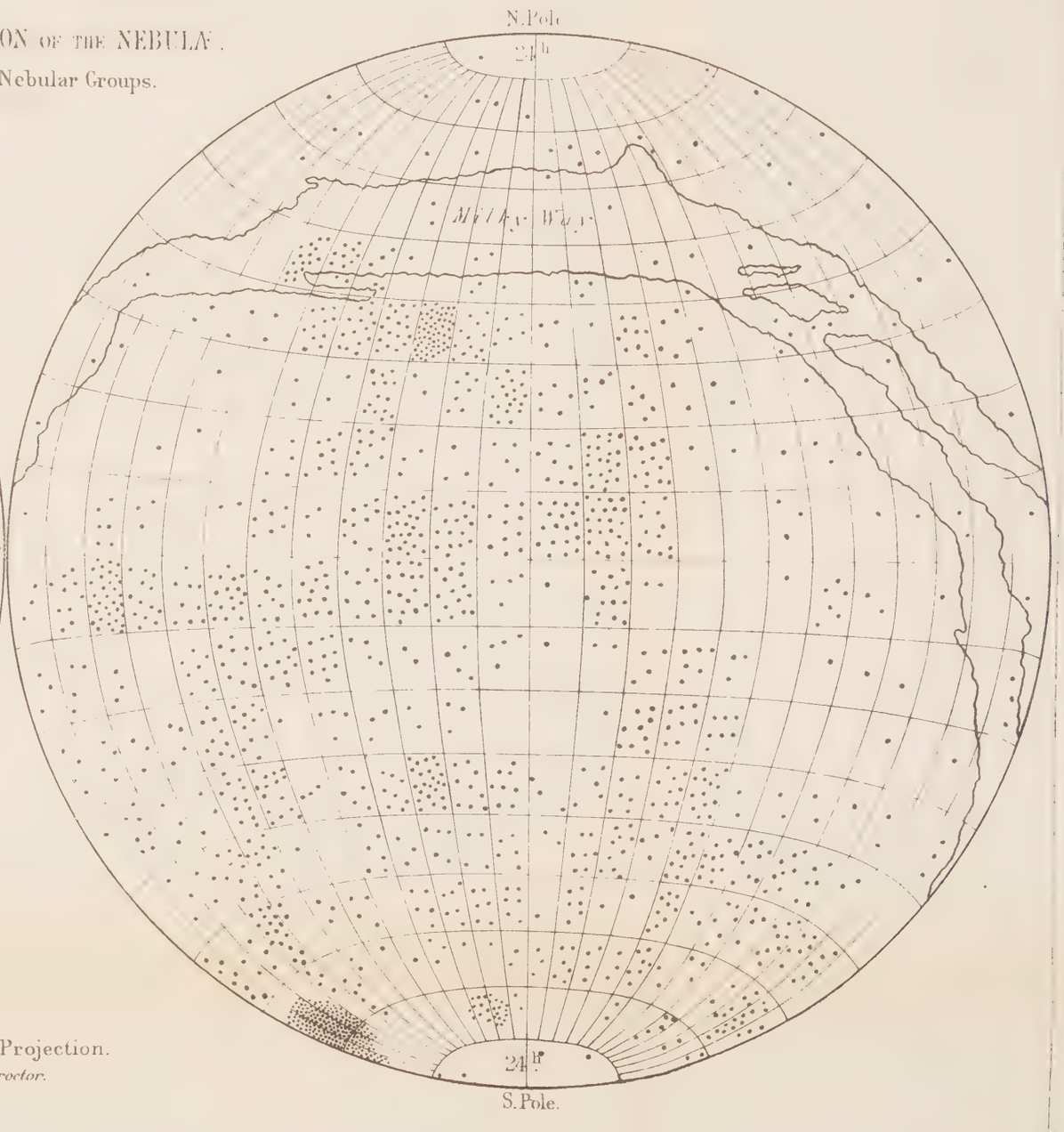
Isographic Projection

R. A. Proctor

II. DISTRIBUTION OF THE NEBULÆ.

The Great Nebular Groups.





Isographic Projection.

R. A. Proctor.

Malby & Sons, Lith.

NORTHERN HEMISPHERE.

SOUTHERN HEMISPHERE.

1. DISTRIBUTION OF THE NEBULÆ.

The Northern & Southern Nebulæ.

Isographic Projection.

R.A. Proctor.

Malby & Sons, Lith.

the star's disk, it is clear that along that quadrant there will be a gradual diminution of light down to evanescence at the extremity. If the star had an apparent diameter only half as large, the arc of fading light would be but half a quadrant, and so on. The observation would be rendered easier by the fact that a neighbouring part of the coil would always give the means of a ready comparison. I find that if the star's image rotated 100 times in a second, a star ten times as far off as *α Centauri*, and having a real diameter as large as our Sun's, would give an arc of fading light about $55\frac{3}{4}°$ in length. Here I suppose the occultation direct. An oblique occultation would obviously be more favourable.

The light of the star would be much reduced by so rapid a rotation; but with a large telescope I believe quite enough light would be obtained even from a star of the lower magnitudes for the purpose in view. Up to a certain point the reduction of light would be advantageous, as it is much easier to detect differences between faint than between brilliant lights.

An observer would require special training for the use of this method, and experimental observations on terrestrial lights would probably be required before any great reliance could be placed on the resulting measurements of stellar disks.

If reliable measures could be obtained however, the results would obviously be most important. We should learn what relations subsist between the real and the apparent magnitudes of stars, and thus what varieties there may be in the intrinsic brilliancies of these orbs; we should have a ready means of detecting the close duplicity of many apparently single stars; and we could not fail to obtain valuable information on other points of interest. One of the chief ends which the successful application of this method would subserve would be to afford us information as to the possible extinction of light in traversing the inter-sidereal spaces.

As respects this last point I would suggest that a careful comparison of the spectra of faint stars with those of the brighter orbs might indicate the occurrence of extinction; since there is reason for anticipating that if extinction really takes place in space there would be signs of *elective* absorption. If certain portions of the spectra of the smaller stars were found to be relatively fainter than the corresponding portions of the spectra of the brighter stars, or if a tendency to such a relation were noted on the average (for we must not forget that many small stars may be near to us), we should have a strong argument in favour of the theory that light is extinguished in the course of its progress through space.

Sept. 11*th*, 1869.

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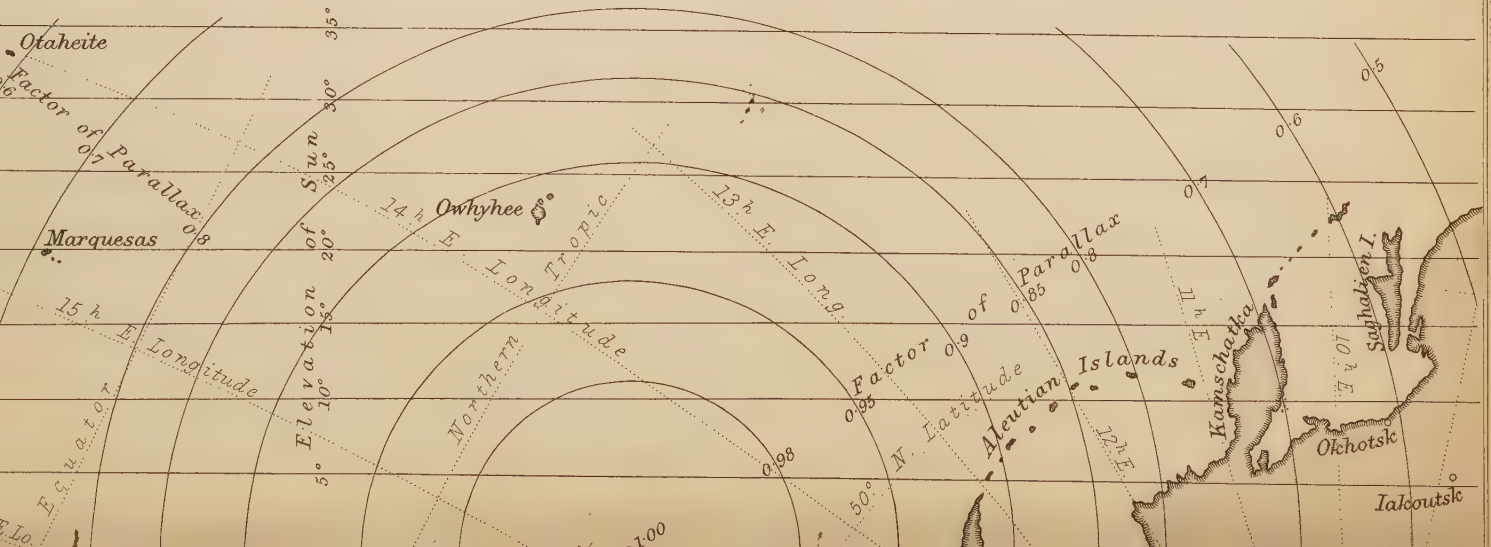
Transit of Venus, 1874, Dec.r 8.

Stations for observation of the Ingress of Venus.

The Sun descending. Venus entering on the Upper Limb.

Ingress accelerated by Parallax.

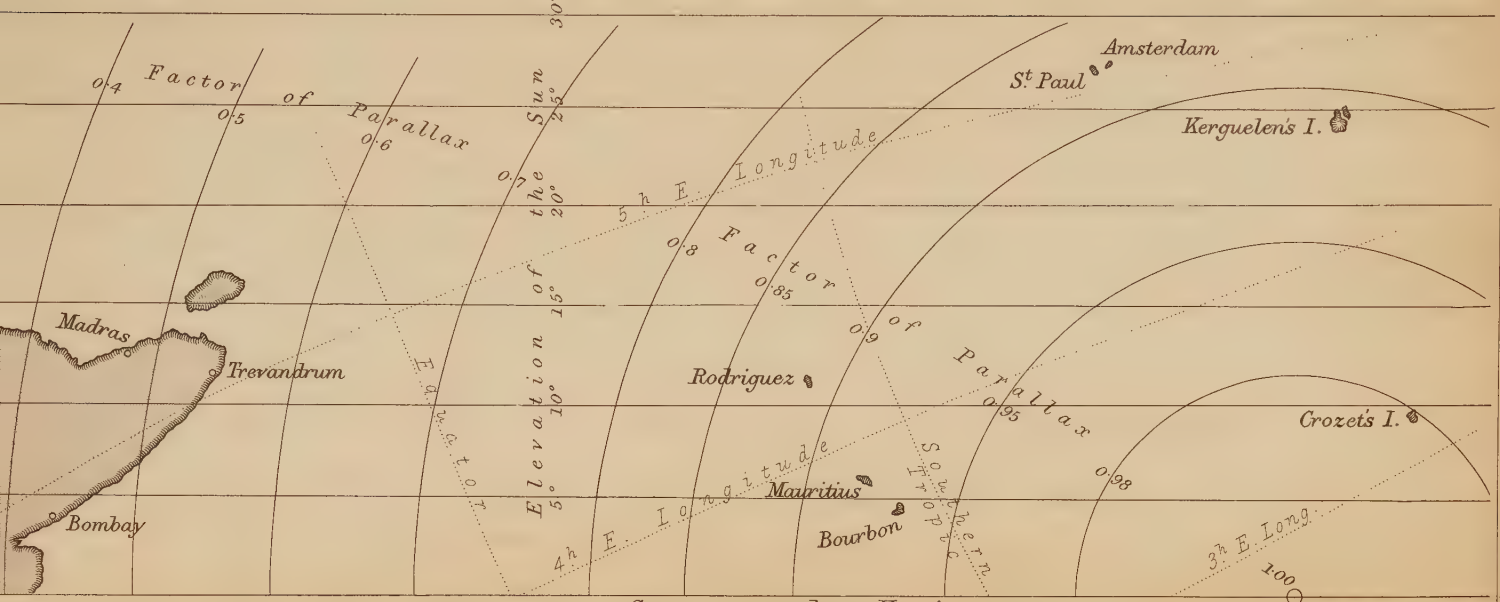


Transit of Venus, 1874, Dec.r 8.

Stations for observation of the Ingress of Venus.

The Sun ascending. Venus entering on the Lower Limb.

Ingress retarded by Parallax.



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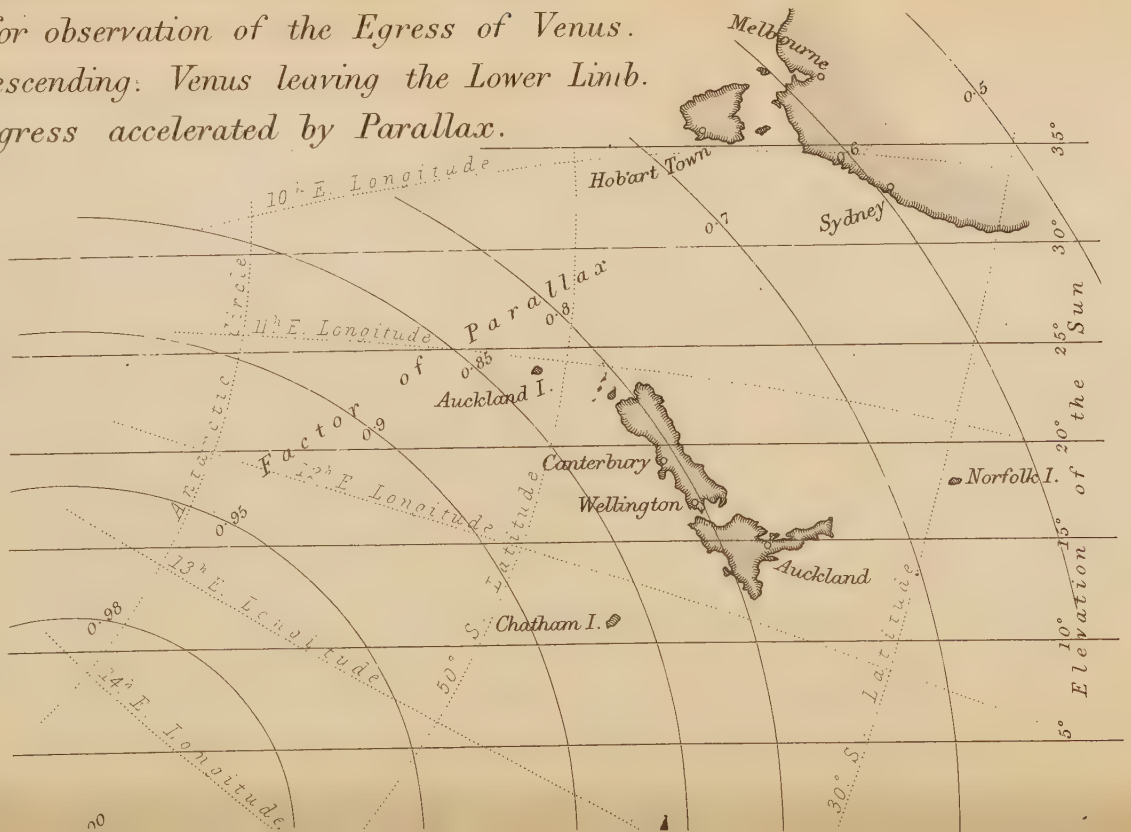
The Sun descending: Venus leaving the Lower Limb.

Egress accelerated by Parallax.



Stations for observation of the Egress of Venus.

The Sun ascending. Venus leaving the Upper Limb.

Egress retarded by Parallax.

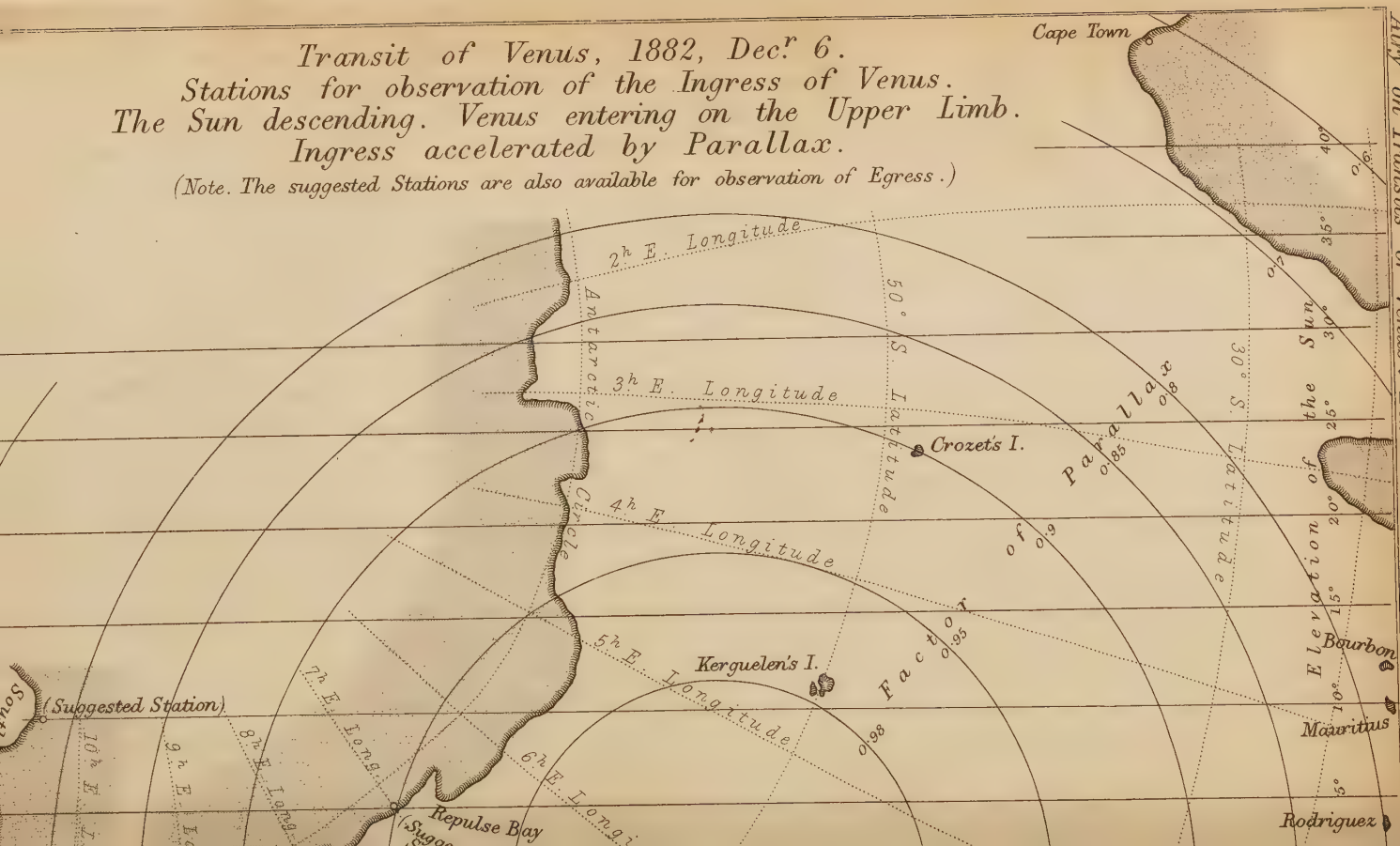
Transit of Venus, 1882, Dec.r 6.

Stations for observation of the Ingress of Venus.

The Sun descending. Venus entering on the Upper Limb.

Ingress accelerated by Parallax.

(Note. The suggested Stations are also available for observation of Egress.)

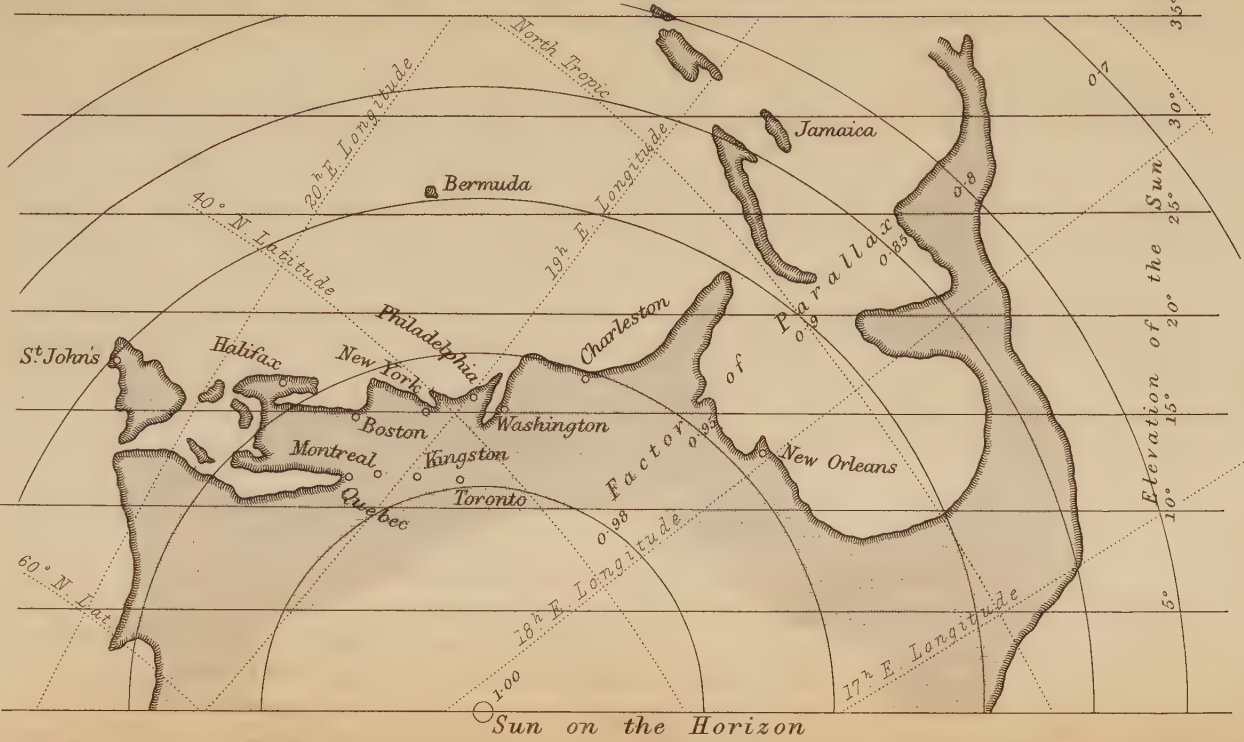


Stations for observation of the Ingress of Venus.

The Sun ascending. Venus entering on the Lower Limb.

Ingress retarded by Parallax.

(Note. The American Stations (except Newfoundland), and the Islands, are also available for observation of Egress.)

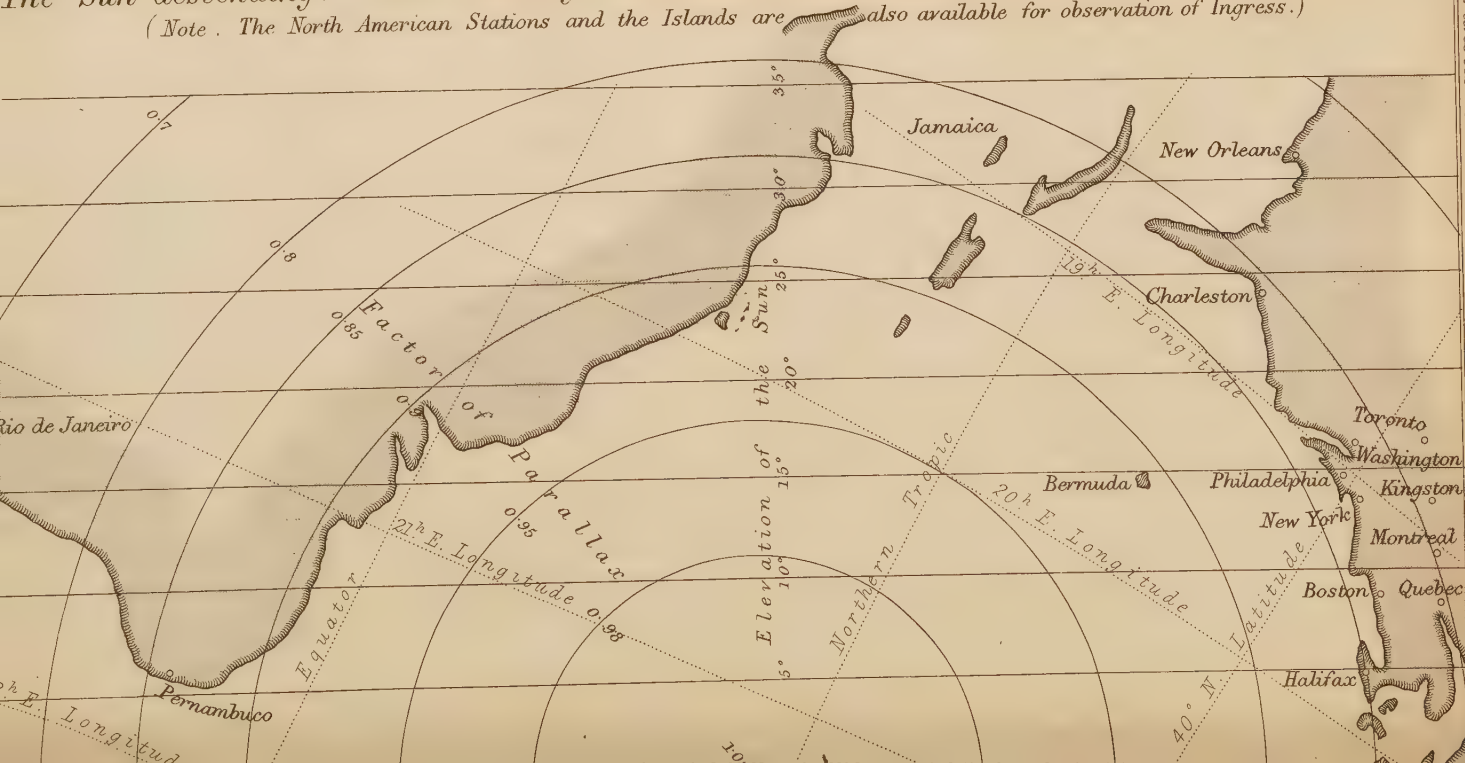


Transit of Venus, 1882, Dec.r 6.

Stations for observation of the Egress of Venus.

The Sun descending. Venus leaving the Lower Limb. Egress accelerated by Parallax.

(Note. The North American Stations and the Islands are also available for observation of Ingress.)



The Sun ascending. Venus leaving the Upper Limb. Egress retarded by Parallax.

(Note. The "Suggested Stations" are also available for observation of Ingress.)

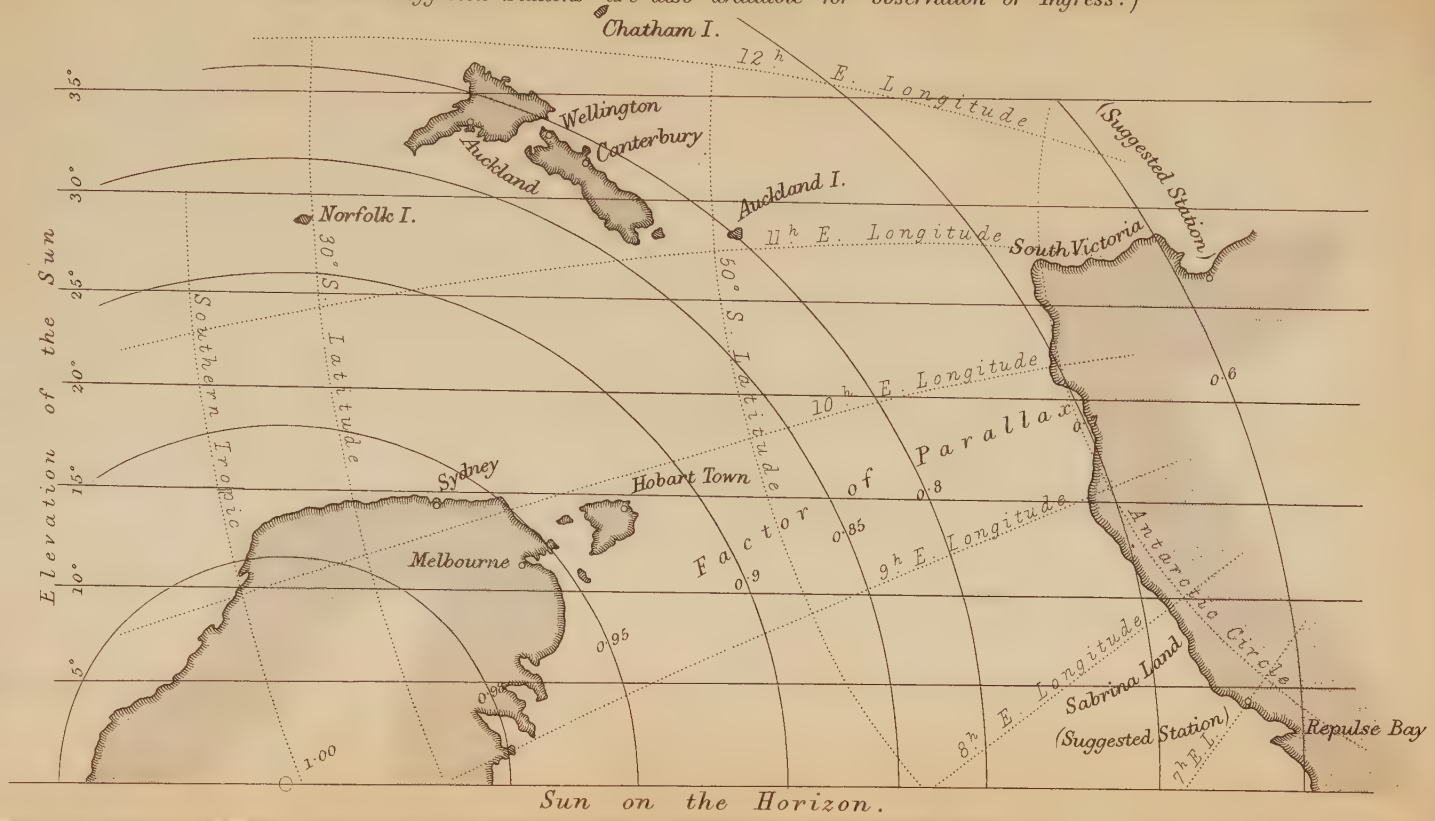
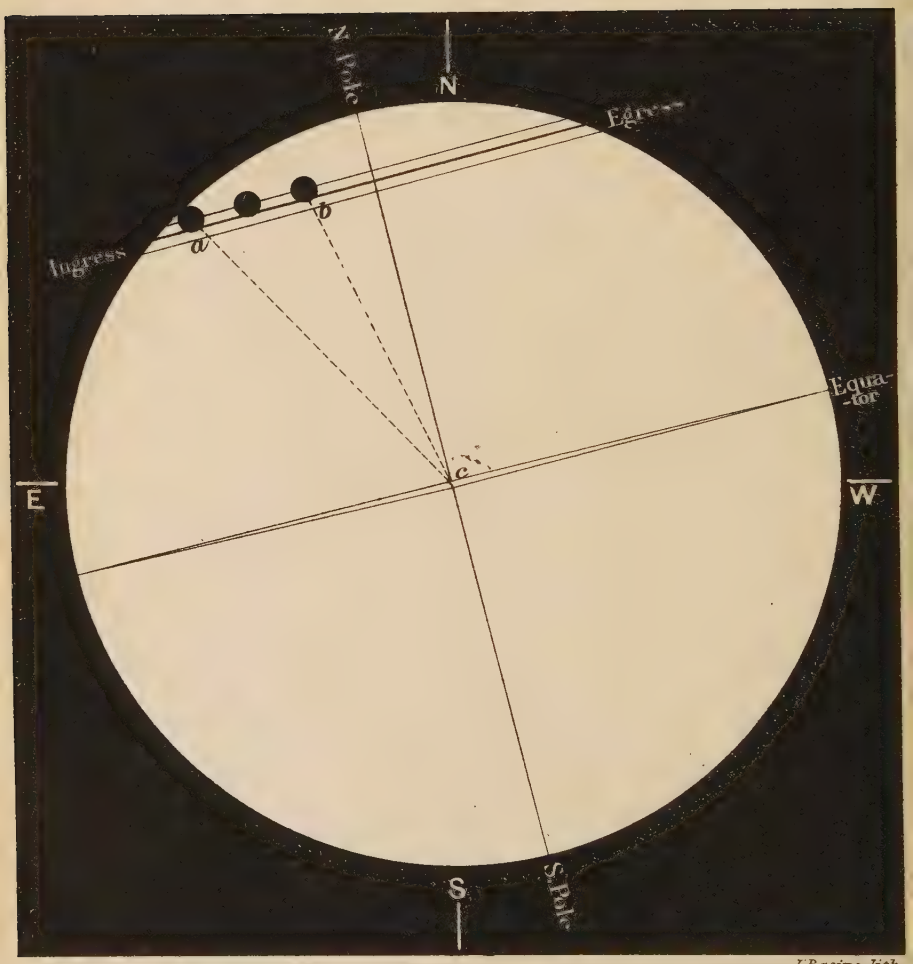
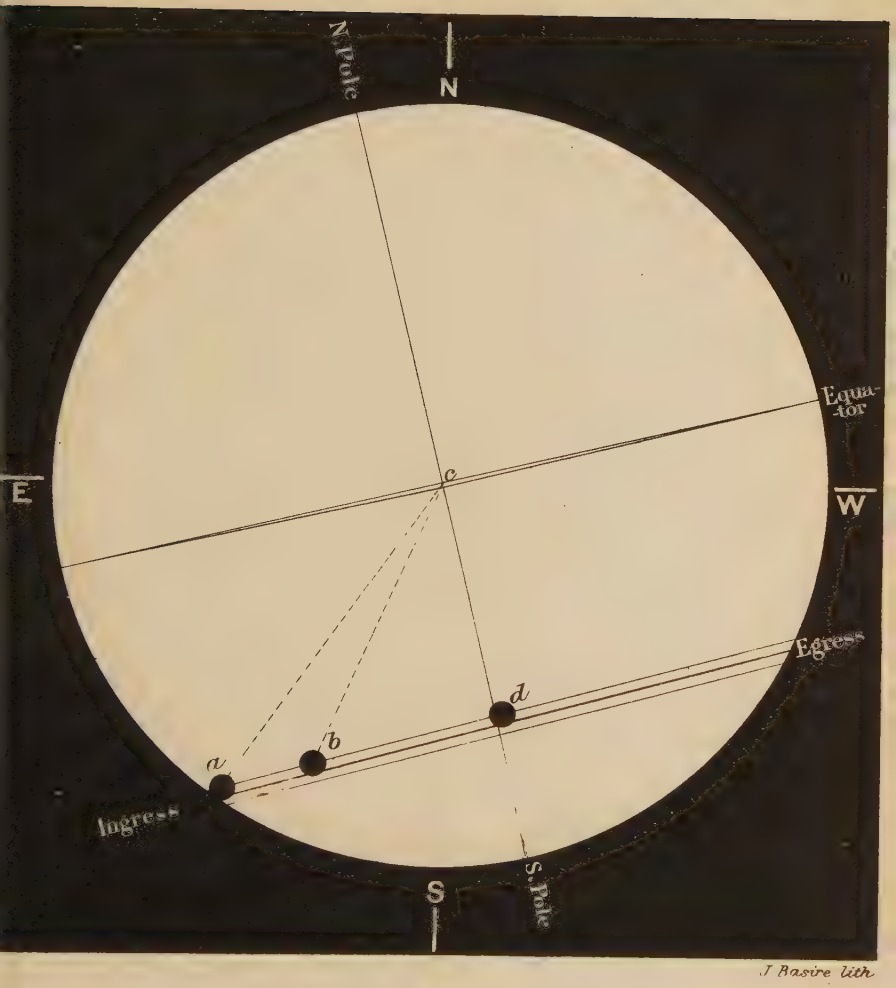


Figure 1.
1874.



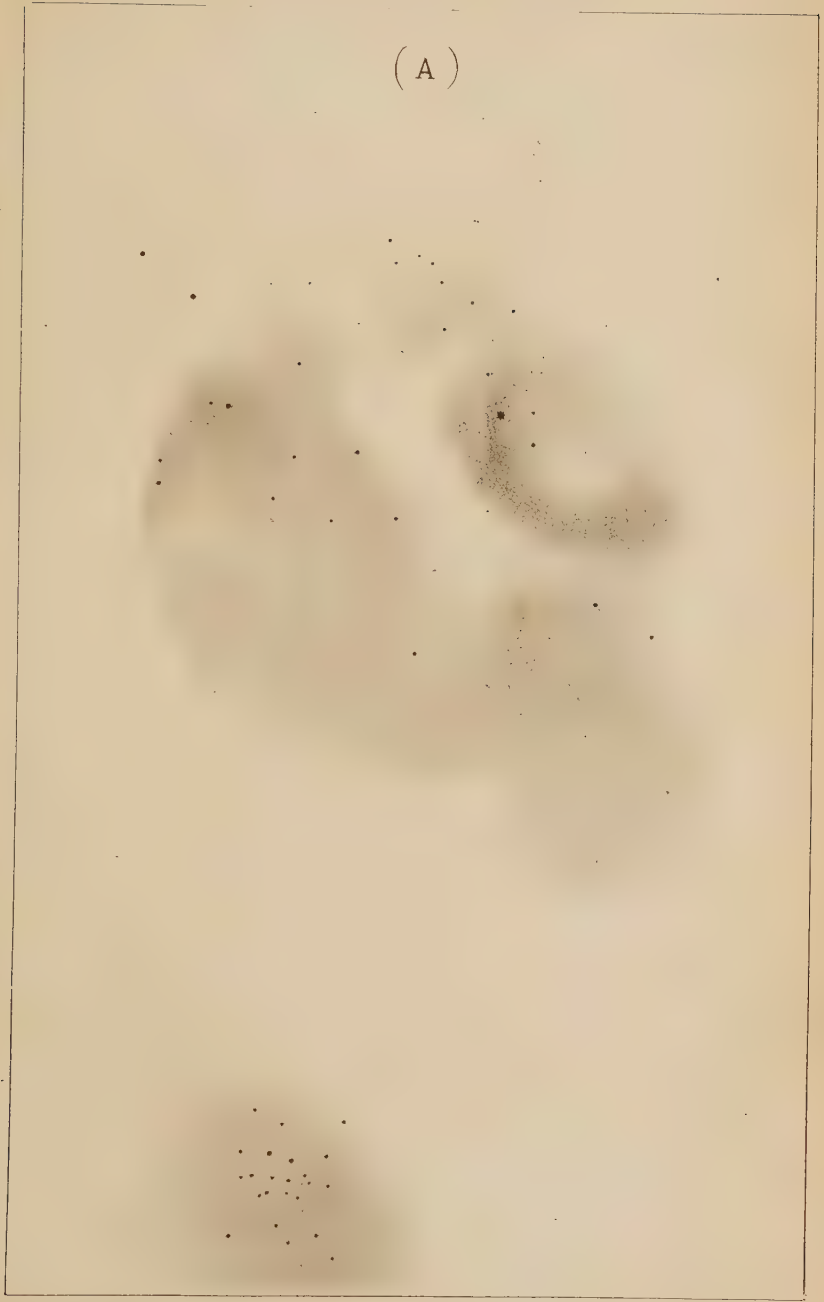
J. Basire lith.

Figure 2.
1882.



J. Basire lith

The Nebula round η Argûs, I.



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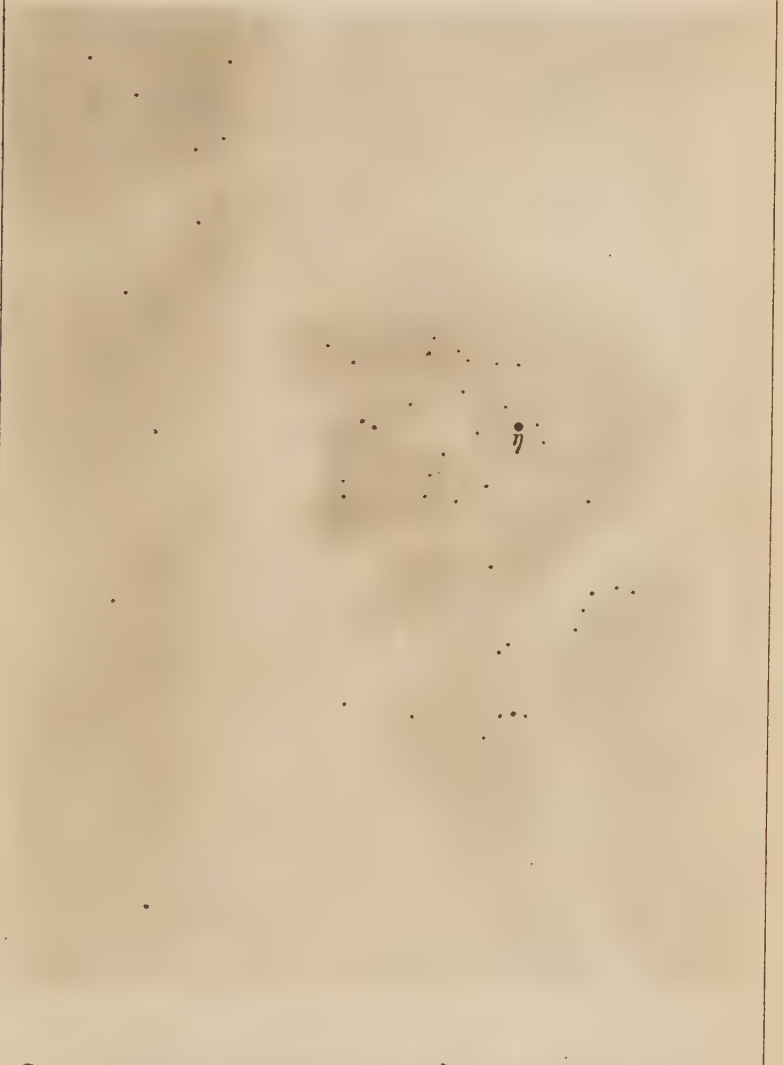
Approximate Length of One degree

1. GENERAL APPEARANCE OF THE NEBULA ARGÛS AS SEEN NOV^{R}, 23RD & 24TH, 1868, WITH APERTURE 5 INCHES, AND MAGNIFYING POWER 55.

Malby & Sons, Lith.

The Nebula round η Argûs, III.



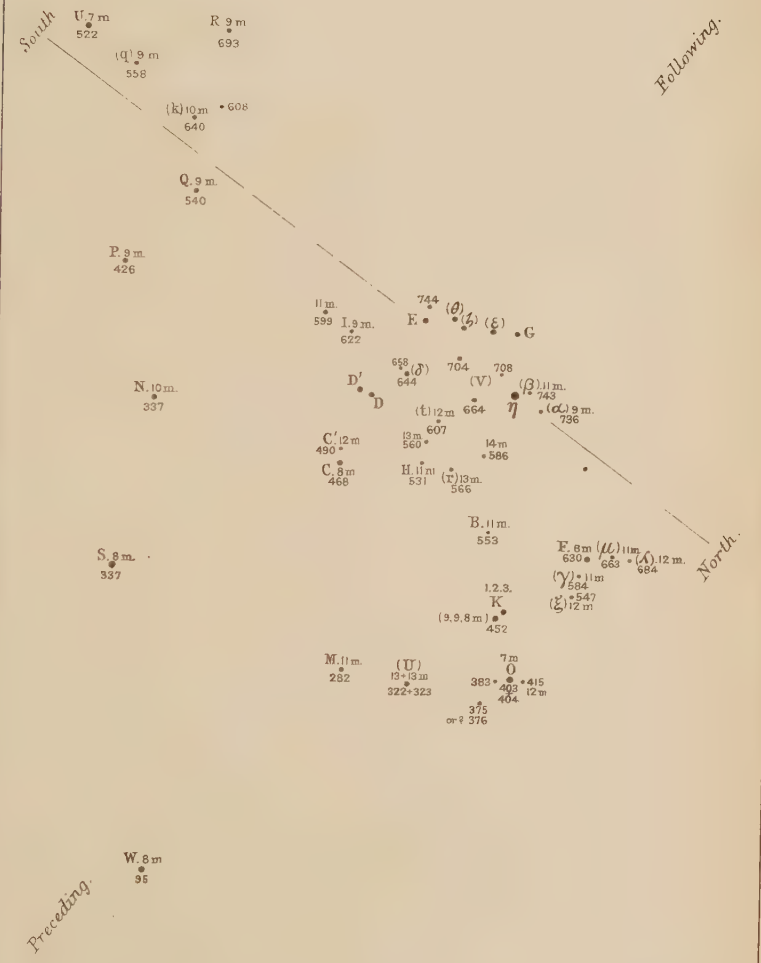
2. A PORTION OF THE NEBULA ARCÛS COPIED FROM SKETCHES AT THE TELESCOPE: APERTURE 5 INCHES: EYEPIECE 125.

J.H. 24/11/68.

Malby & Sons, Lith.

The Nebula round η Argûs, IV.

J.F.W.H. (A.A.).

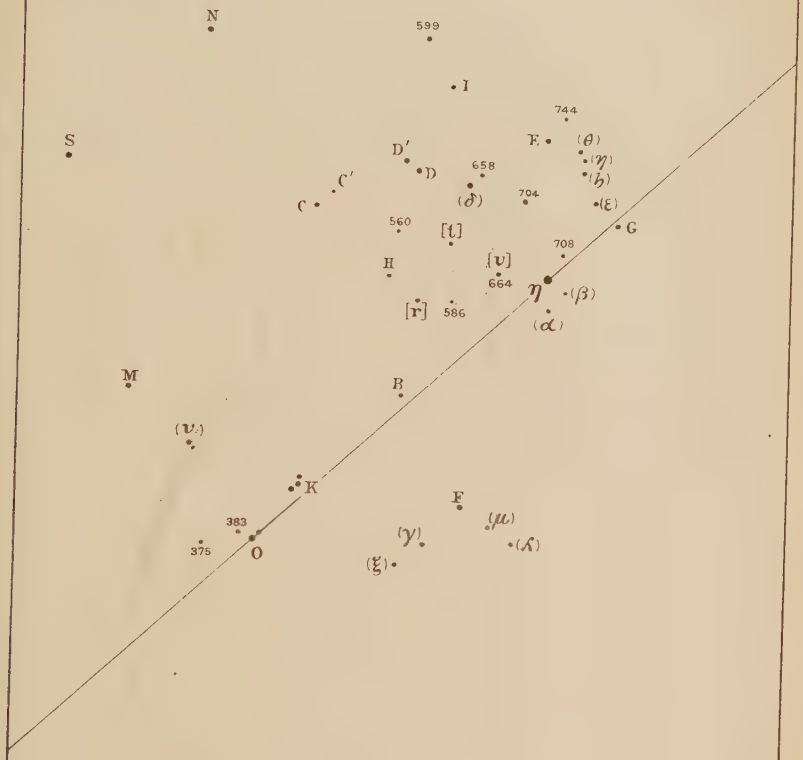


IDENTIFICATION OF STARS PRICKED OFF FROM LIEUT. J. HERSCHEL'S DRAWING, MARKED 2 OF THE 24TH NOVR, 1868.

Malby & Sons, Lith

The Nebula round η Argûs, V.

J.F.W.H. (B.B.).



SEMIREVERSED BY PRICKING OFF FROM SIR J. HERSCHEL'S ENGRAVING OF THE NEBULA SHEWING THE TIME AND SITUATION OF THE IDENTIFIED STARS IN LIEUT. HERSCHEL'S SKETCH OF NOVR. 24TH, 1868, AND THE LEADING LINES OF THE NEBULA AMONG THEM, AS IT EXISTED IN 1834 — 1838.

The Oblique Line through η & O corresponds to the upright Sides of Lieut. Herschel's diagram.

Malby & Sons, Lith.

John Browning, del.

TRAIN OF SUN-SPOTS.

Fig. 1.

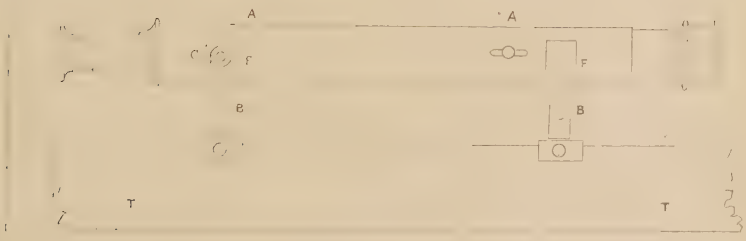


Fig. 2.

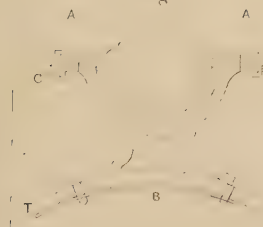


Fig. 4.

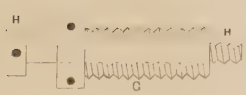
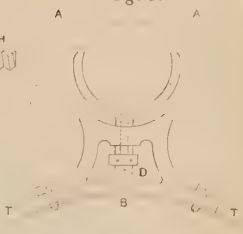


Fig. 3.



NEW MOUNTING FOR FINDER.

John Browning, del.

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